

ART. X.—*On the Etiology of Intermittent and Remittent Fevers.*—  
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THE etiology of intermittent and remittent fevers has long engaged the attention of the profession, but though repeatedly investigated by some of the ablest minds, it is still to a certain extent involved in mystery. The first who put forth definite views on the subject was John Maria Lancisci, an Italian. In a work entitled "*De noxiis Paludum Effluviis*," printed in 1717, he advanced the idea that these fevers were caused by certain marshy exhalations, to which he gave the name "*miasmata*"—from the Greek word *μιασμα*, to pollute. But subsequent observation showed that the vicinities of marshes and stagnant pools were not the only places where the disease was met with. It was found where no marshes existed; and, as the Fall of the year was the season when the disease was most prevalent, it was attributed to a poison generated in the decomposition of vegetable matter. This view was generally adopted by the profession until the publication of Dr. William Ferguson's paper "*On the Marsh Poison*," in the *Edinburgh Philosophical Transactions* for the year 1821. This eminent physician, during his connection with the medical staff of the British army in the Peninsular war, collected many interesting facts on the subject. He showed that vegetable decomposition was not necessary to produce the disease;—"that the peculiar poison may prevail where there is no decaying vegetable matter, and no vegetable matter to decay; and that for producing the poison all that is necessary is a surface capable of absorbing moisture, and that this surface should be flooded and soaked with moisture and then dried; and the higher the temperature, and the quicker the drying process, the more plentiful, and the more virulent, is the poison that is evolved."

Some have asserted that the miasm is a *halitus* from the bowels of the earth which escapes through the fissures produced by the warmth of the sun, after heavy rains in the summer and autumn months. Facts, it appears to me, are constantly met with which none of these theories can explain. Let us examine them a little more in detail.

First: *The theory of vegetable decomposition.*—The disease attributed to its effects makes its appearance usually about the first of August and continues until about the middle of October, or the appearance of frost. This may be taken as the general law, but it varies in different latitudes. It is met with again in the Spring, during the latter part of April, the whole of May, and running on sometimes through the first and second weeks of June. Let us trace vegetable decomposition through the same months and see if it can be the cause of all this mischief.

During the latter part of July, the whole of August and the early part of September we have very little of it. Even the appearance of decay is scarcely perceptible. The grain and hay stubble, the dried stems of plants whose process of fructification is completed, are all that can be undergoing the process then:—and they decompose very slowly. In the Spring they are often found very little changed; but after the appearance of the early frost in October, the sun coming out warm through the day, life being arrested while the stems are succulent, there is a supply of moisture present, and the process of decomposition goes on in the day-time with great rapidity. If the miasm causing the disease is produced by the decomposition of vegetable matter we ought to have it then in a *maximum*, and the disease in

its most malignant form. But is such the fact? Everybody who has lived in a malarious region, as it is called, knows that frost is hailed by the inhabitants as a deliverer. That after a severe frost, the influence of which can be felt and seen, the disease is mitigated or entirely arrested. As the Fall advances the decomposition of vegetable matter increases, and is kept up in southern countries through the Winter. But it is a well-known fact that the course of the disease is just the reverse. In April and May nearly all vegetable decomposition has ceased. All that can be undergoing the process then are some plants of a very fibrous texture, that have resisted the action of nature during the preceding Fall and Winter, and those vegetable remains that are now applied as manures, the elements of which are soon appropriated by the growing plants to their nourishment. Certainly the miasm ought now to be at a *minimum*. Yet we have at that time, in malarious neighbourhoods, a good deal of intermittent fever.

Dr. Ferguson has given us, in his paper "On the Marsh Poison," many instances of the prevalence of intermittents and remittents in places where vegetation was not only absent, but its existence absolutely impossible, as along the dried bed of the Guadiana, the dry sandy surface on the Alantejo side of the Tagus opposite Lisbon, and a "bare open, hollow country like the dried up bed of an extensive lake" near Ciudad Rodrigo.

But the decomposition of vegetable matter goes on abundantly without the production of the miasm. "The rotting cabbage-leaves of Covent Garden, and those which taint the air of the street from the neglected dust-holes of London, during the hot weather of Summer, give rise to no ague. The same may be said of the putrefying and offensive sea-weed, which is deposited in large quantities upon some very healthy parts of our sea-coast" (*Watson's Practice*, p. 454).

In the west of Ireland the farmers are in the habit of collecting the algæ along the sea-coast, every year, and spreading it over their farms as manure. This is done in the months of July and August, when the temperature is highest. It undergoes decomposition very rapidly, emitting a most abominable stench. A gentleman who spent some time at a watering place near Ballyshannon, a few years ago, informed me, that he has seen the fields covered with it for miles; yet no intermittents follow. The same fact has been corroborated by another gentleman, now at Princeton College, whose father owns property along the north-west coast of Ireland.

Sir James Clarke, in his work on the sanative influence of climate, says: "It is remarked in the statistical reports on the health of the navy, that on the South American station there are land-locked harbours, where, under a powerful sun, ships lie for months, or years; surrounded by a country abounding in marshes and rank vegetation and all the other circumstances which elsewhere are considered the essential cause of the fevers which prove so destructive of life among Europeans, without the occurrence of a single case of fever. 'The crews on the contrary enjoying good health.'—(p. 171, Philadelphia ed.)

"The town of Point au Petre in Guadaloupe is situated among the most putrid marshes in the world, the stench of which is never absent from the streets; yet the place is far from being uniformly unhealthy. Strangers, though much annoyed by the smell, often resorted to the place with impunity." (*Ferguson on Marsh Poison*, p. 10.)

There is "a large cess pool to the windward of Belfast (Ireland), which is the reservoir of several sewers from neighbouring streets, from a house of correction, a lunatic asylum, and from a great number of cotton factories, a distillery, and other extensive sources of animal and vegetable deposits

and impurities. This has an accession of brackish water from the oozy Lagan, the principal river of the port, at every tide. Notwithstanding the emanations and fetid gases from that effervescing fen are constant, they do not engender agues." (*Sir J. Murray in Dublin Med. Press*, Nov. 1844.) Fort Moultrie, at the mouth of Charleston harbour, is another example. "Although there is much salt water marsh in the rear of the island on which it is situated, no deleterious effects arise." (*Medical Statistics of U. S. Army*.) Similar examples are found in Forts Brady, Howard and Winnebago; while at Forts Niagara, Washington, and Bellona Arsenal, where no marshes exist, these fevers are prevalent.

Some have dwelt much on the agency of putrefying animal matter in producing the poison; but the argument is more specious than solid. Dr. James Johnson, who resided many years in British India, and who devoted much attention to these diseases, in speaking of their causes, says, "that during the month of May and beginning of June, when the rivers are sunk far within their autumnal boundaries, when the heat is excessive, and when the tides are so rapid that the *bore*, as it is called, rushes up past Calcutta, sometimes with the amazing velocity of *twenty miles an hour*, not entirely stopping till it reaches Niaserai, thirty-five miles above the capital; then indeed, at low water, each side of the river presents a broad shelving slope of mud and mire, covered with vegeto-animal remains in all stages of putrefaction and disengaging the most abominable stench, yet no ill effects whatever are produced by such exhalations." (*Tropical Climates*, p. 107.)

In view of all these facts, we think we may safely doubt the tenability of the theory of vegetable and vegeto-animal decomposition. On the contrary we see that as a country becomes improved and vegetable production—and in consequence vegetable decomposition—is increased, the disease diminishes.

Let us next examine the theory of Ferguson, viz., that the poison is generated in the rapid drying of a porous sandy surface:—or, in other words, for producing the miasm it is requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water and then dried; and the higher the temperature and the quicker the drying process, the more plentiful and virulent is the poison that is evolved.

The distinguished author, from his connection with the medical staff of the British army during the Peninsular war, enjoyed an extensive field for observation; and his subsequent appointment on the medico-topographical survey of the West Indies, during the years 1815, '16, and '17, gave him diversified opportunities for adding to, and verifying, the observations he had previously made, all of which he sedulously improved, and has given us as the result of his labours a number of facts on the subject, as valuable as they are interesting. It would make this paper too long to insert them here. Neither do we think it necessary for our present purpose. They all go to refute the theory of vegetable decomposition. But we humbly think that the one he proposes as a substitute is equally objectionable.

These fevers prevail only in certain seasons, generally in Fall and Spring. Is not the soil of places where they do prevail equally as wet during the latter part of June and the whole of July—and exposed to suns equally as hot, and consequently have the drying process taking place as rapid as in August, September, or October,—April or May? We think more so.

In July, especially, rains are more frequent than in either August or September.

The Spring and early part of the Summer of 1847 were unusually dry throughout lower Virginia.\* The drought prevailed to an alarming extent. Vegetation was parched. On the 19th and 20th of June there was a copious rain, soaking the ground thoroughly. The following week the weather was oppressively hot,—the thermometer at 2½ o'clock standing from 92° to 94° Fahr. in the shade, except on the 24th, when it stood at 81°. Here the ground was thoroughly wet and followed by intense heat, producing, of course, rapid evaporation; yet not a case of fever followed. Whereas, during May, when the whole country around was suffering from drought, and the thermometer in only a few instances rose above 80°, chills and fevers were very frequent. Other facts might be adduced, but these, we think, are sufficient to prove that rapid evaporation from the soil of places where these fevers prevail, and the disease, cannot be connected as cause and effect.

The same objection, we think, might be urged against the view that the disease is produced by a *halitus* from the bowels of the earth. What makes this *halitus* innocuous during June and July, and so active in August, September, and October? The fissures, through which it is said to make its escape, we have seen as abundant at the one time as at the other.

These anomalies compel us to doubt the tenability of all the above theories. We think they cannot be sustained by an appeal to facts. And the more the attention of the profession is directed to the subject, not in one place or neighbourhood, but in many dissimilarly situated, the number of these, we think, will increase.

While a student of medicine, I resided between three and four years in a miasmatic district. During the first fall of my residence there, intermittent and remittent fevers prevailed to an unwonted extent. Very few in the neighbourhood escaped. But I observed that the few who did escape were among the labourers on the plantation; and those of them that were taken with the fever had it much milder than the household servants. This circumstance appeared to me, at first, inexplicable. These persons were out before sunrise, and did not return to breakfast until between 7 and 8 o'clock. They were out regularly, also, until after sun-set. Their work lay along the shore of the river, where the miasm is supposed to be most abundant. Being out so early, and with empty stomachs, they were certainly more exposed to the poison than the household servants, and ought, reasoning *à priori*, to have the disease in a severer form. Yet, taking them as a class, comparatively speaking, they escaped. I observed the same circumstance the following Spring and Autumn. This led me to doubt the correctness of the conclusion that these diseases are produced by a specific poison; and to look for their cause in other agencies.

Among these, as the most important in all natural changes, we first look to the effects of temperature. Heat acts as a stimulus to the organic functions of the body. The annual changes that take place in the vegetable world, and the superior luxuriance of the vegetation of warm climates as compared with cold, illustrates this in reference to plants. Similar is the effect produced on those functions which man possesses in common with plants. The liver and the skin, from the nature of their functions, especially are stimulated by heat. The lungs and liver are the

\* The writer was at that time a resident of lower Virginia.

great decarbonizing organs of the body, and the activity of their functions is always in an inverse ratio. In the lungs, carbon undergoes slow combustion, accompanied by a disengagement of heat for keeping up the animal temperature and is thrown off as carbonic acid. In the liver, it unites with hydrogen, and small portions of oxygen and nitrogen, and forms bile. In summer, on account of the high temperature of the air, there is less demand on the calorific apparatus within the body; and the atmosphere, on account of its rarity, contains less oxygen in the same volume than in winter. These two causes co-operating, will diminish the quantity of oxygen taken into the system by respiration, and, in the same proportion, the quantity of carbon thrown off as carbonic acid. Hence there will be more labour thrown on the liver. For the performance of this, in accordance with the general law of secretion, increased activity of the circulation through the portal system will be required. Of this activity, all the radicals of the large veins that unite to form the vena portarum must, more or less, partake. The circulation must therefore be more active in the digestive organs during summer than in winter. Its activity is also much increased after a meal, because an increased supply of blood is then demanded by the digestive organs.

The activity of the function of the skin is also much increased during summer. The function of the cutaneous surface is twofold, secretory and respiratory. As a product of its secretory function, we have perspiration and a sebaceous matter for keeping the skin soft and in a proper condition for the healthy performance of its functions. Besides this, a large quantity of fluid passes off, by simple evaporation, exceeding by far that secreted by the sudoriferous glands. (*Carpenter, Principles of Physiol.*, sect. 701.) The respiratory function of the skin, though not capable of sustaining life in an animal of such active vitality as man, is by no means inconsiderable. In some of the lower tribes of animals, indeed, it is a very important part of their respiratory process; and even in some vertebrata the cutaneous respiration is capable of supporting life for a considerable time. This is especially the case in batrachia, whose skin is soft, thin, and moist. Experiments which have been made on the human subject leave no room for doubt that a similar process is effected through his general surface.

The skin, therefore, by increased function during summer, keeps the temperature of the body down to the proper point by means of its secretory power, and the consequent evaporation: while it assists the liver in the decarbonization of the blood by means of its respiratory power. The manner in which these processes will be performed will depend much on the state of the dew-point—a high one interfering with, and a low one favouring, their proper performance. This brief explanation of the functions of the liver and skin may help to account for that sympathy that has been observed to exist between them.\* Heat acts as a stimulus to both. Now we know that whenever a stimulus is applied to an organ, one of two things must, in general, ensue. If the stimulus applied be constant, and the excitement sufficiently kept up for any length of time, serious injury is likely to ensue to the organ itself, even so far as alteration of structure. But if the application of the stimulus be only temporary, or the force not in any great degree, then a reaction follows the excitement, and we have an exhausted state of the organ, producing diminution of its function, and susceptibility to the action of any depressing agent that may be brought to bear on it.

\* Johnson "On Tropical Climates," p. 19, 242. Forrey on Climate, &c.

In Summer the liver and skin are stimulated during the day. In June and July the temperature of the atmosphere does not fall low enough at night to act as a sedative on the system, or to act positively on the system at all. During the summer of 1847, I kept a journal of the thermometer through these months, in a malarious district, and found the average temperature at eleven o'clock at night to be 69° Fahr. This temperature, as a general rule, would exercise no positive influence on the system; so that, although the liver is stimulated through the day by the heat, no evil results follow. It only experiences that state of reaction which always follows a period of excitement similar to what the rest of the body experiences after the labours of the day; all of which is removed by the refreshing influences of sleep.

In August, September, and October, while the heat of the day remains nearly the same as in the two preceding months, the average temperature immediately before sunrise is under 50°, and at eleven o'clock at night is very little above it. This change takes place very rapidly after sunset; and the effect will be very different from that experienced in July. The skin and liver being in a state of exhaustion from the previous excitement, will be very susceptible to the influence of the sudden change; and its obvious effect will be to drive the blood from the surface on the internal viscera, producing temporary congestion. The liver, on account of its previous activity being now the most exhausted organ of the viscera, will be likely to suffer most. And if the previous stimulation has been great, and the depressing agent sufficiently intense, the patient may have an attack of acute bilious fever. But if the causes act slightly, the tonic of the system being regained by sleep and rest, the viscera will be enabled to restore the equilibrium of the circulation before the temporary congestion will cause any irritation. The liver, however, from having the greater amount of the congestion thrown on it, may not be able, by its restored tonic, to press out the congesting fluid entirely, and parts of the organ will remain in a state of partial congestion. This, according to the degree in which it exists, may or may not interfere perceptibly with the functions of the organ. If the patient is not again exposed to the depressing influence of cold at night, the "*vis medicatrix naturæ*" will correct the effects of the preceding exposure (if they have been slight), and no ill results follow. But if he be repeatedly exposed to the same influences, every successive exposure will increase the existing morbid condition, and the functions of the organ will soon become obviously deranged, involving also, necessarily, the functions of the large and small intestines and stomach. He will have a bitter taste in his mouth on getting out of bed in the morning, furred tongue, yellow adnata, constipated bowels, languor, a dull heavy feeling over the eyes. The function of the skin by virtue of the "*cutaneo-hepatic sympathy*," mentioned before, becomes involved in the morbid condition. The surface is pale, dry, and sallow.\* If the patient now take a purge of some medi-

\* Dr. Samuel Jackson, Professor of the Institutes of Medicine in the University of Pennsylvania, has given many interesting facts to prove that nerve power is a modification of electricity. This electricity, he supposes, is generated in a vital chemical action in the union of an acid with an alkali, in the animal economy. Thus the blood is alkaline, and the juices of the tissues are acid, chemical action is the result, and currents of electricity, or nerve power, are evolved. The bile and the fluid secreted by the skin have the same relation. Now Dr. James Johnson has, we think, established the fact, that a sympathy exists between the liver and the skin, the secretions of both being in-

cine that acts on the liver, it will relieve the hepatic congestion, and he will be well. This I have tried repeatedly in my own case, and many times in others, and never knew it to fail. But if he continue to be exposed to the same influence as before, the hepatic derangement will go on increasing, the tonic of the organ constantly becoming less, until at last, during one of the periodic determinations of blood to the part, before explained, it gives way; and the circulating fluid receding from the surface, collects in the liver, spleen, and portal vessels, in abnormal quantities, and we have the phenomena of a chill. The blood here soon acts as an irritant; reaction takes place, tonic returns with excitement, and we have fever. If the previous congestion was not sufficiently intense to excite inflammation, the fever after a while passes off, and the patient has an *intermittent*. But if inflammation has been excited, or the irritation caused by the congestion continues, the fever only abates after the period of excitement, and the patient has a *remittent*. From the relation the liver sustains to the stomach, spleen, small and large intestines, its functions cannot be much deranged without the latter organs becoming affected, and it is easy to see how any of them may become involved in the course of the disease.

From the various modifications of the cause, the disease is met with in various degrees of malignity. From the greater length of the days at the solstice in this latitude, than nearer the equator, the approach of the season of cool nights is more gradual, and the change is seldom decidedly felt before the autumnal equinox. The disease is, therefore, more gradually produced, and later in making its appearance, than it is farther south. The system, to a certain extent, accommodates itself to the progressing lesion, and the morbid condition may be greater before the disease declares itself, and the paroxysm less violent than it is in places where the causes act with more power. May not this account for the obstinacy that intermittents sometimes present in this climate?

As we go south, the days become hotter on account of the sun's rays being more vertical. The dew-point also rises, and the power of the chemical rays is much increased. The nights are, relatively, cooler. As a consequence we have the disease in a more severe form. Farther south still, we have the causes increasing in intensity, and the disease in malignity. The *remittent* becomes there the prominent type. In Pensacola, New Orleans, and other places along the Gulf of Mexico, where the inhabitants are exposed during the day to the intense heat of a tropical sun, the hepatic stimulation must be very great; and the activity of the portal circulation must be increased in a corresponding degree. As a necessary result, the liver is in an exhausted condition, and the digestive organs have a predisposition to take on inflammatory action. But the nights are so cool as to make a blanket necessary to secure comfort; and the depressing influence of this low temperature is increased by the atmosphere being surcharged with moisture; variations in the dew-point in these latitudes being often as much as 30° or 35° in the twenty-four hours. When these agents combined come to act on the system in the condition in which it is left by the high temperature to which it is exposed throughout the day, the congestion produced must be very great, extending to the intestines and stomach; inflammation is excited in these organs, and we have *yellow fever*.

creased or diminished in the same ratio. In the condition of system mentioned above, both are diminished. The generation of electricity, *nerve power*, will also be diminished. May not this explain that languor which always accompanies this state of things?

Confirmatory of this view is the fact that the first attack of yellow fever takes place often in the night. (*Wood's Practice*, p. 297, vol. i.) The symptoms of the incipient stage of the severe form of remittent and of yellow fever are very much alike: the difference is only in degree, not in kind; and this is not greater than the difference in the intensity of the producing causes would lead us to expect. Yellow-fever, then, we think, is produced by the same causes as intermittent and remittent, but acting with greater intensity. We therefore meet with it only in places where these causes exist in a higher degree than where intermittents and remittents prevail. But on this point we must be more explicit.

It is generally supposed that countries lying under the equinoctial line, or close to it, are subject to the greatest comparative heat, which constantly decreases with the increase of distance from that line. This general rule must be admitted for the countries that lie between the tropics and the poles; but it may be questioned how far it is true of the countries within the tropics. A glance at a globe or sphere, shows that the sun, at the end of the first month after the equinox, has already advanced  $12^{\circ}$  of latitude towards the tropic, but in the second, it traverses only  $8^{\circ}$ . At the end of the second month it is consequently  $20^{\circ}$  from the equator. There remains, therefore, only  $3\frac{1}{2}^{\circ}$  to be traversed in the third month. The sun recedes from the tropics in the same way. It passes, the first month, through  $3\frac{1}{2}^{\circ}$ , the second, through  $8^{\circ}$ , and the third, through  $12^{\circ}$  of latitude. Hence, it is evident, that at all places between  $20^{\circ}$  and  $23\frac{1}{2}^{\circ}$  of latitude, the solar rays during two whole months fall at noon, either perpendicularly or at an angle which deviates from a perpendicular only by  $3\frac{1}{2}^{\circ}$  at most. If we take a place intermediate between  $20^{\circ}$  and  $23\frac{1}{2}^{\circ}$  of latitude, the solar rays must fall on it during two whole months either perpendicularly or in a direction *still less* removed from the perpendicular than in the former case. On the other hand, when the sun is passing the equator, two places on which the vertical rays of the sun fall, on two consecutive days, are nearly  $\frac{1}{2}^{\circ}$  of latitude distant from each other; and a place situated under the equator exactly, has only during *six days* the sun as near its zenith as the above mentioned places, near the tropics, have it during *two whole months*. We might, therefore, presume that the summer heat of the latter position must be much greater than that of places near the equator. This degree of temperature must be still further increased by the greater length of the longest days, which, near the tropics, are  $13\frac{1}{2}$  hours, but at the equator, they are always 12 hours.

This reasoning is borne out by experience. It is much to be regretted that so few intertropical meteorological observations have been published, and of these we know not how they were made, and how they were affected by local circumstances. There is, however, a well-established fact which clearly shows, that in summer the mean temperature near the tropics is higher than at the equator. This is the line of perpetual snow, which, in the Bolivian Andes, between  $14^{\circ}$  and  $17^{\circ}$  south latitude, was found, by Mr. Pentland, by a great number of observations, to be at the height of 17,000 feet, being 1000 feet higher than it is in Ecuador, under the equator. Poëppig, in traversing the Andes near  $11^{\circ}$  south latitude, found that here also the snow line was several hundred feet higher than under the equator. The difference would be still greater in the same degree of north latitude, as all places in the northern hemisphere have a higher temperature, as a general rule, than those situated in the southern at the same distance from the equator. The countries in which the great-



est degree of heat may be expected, therefore, are those that lie between the  $16^{\circ}$  or  $17^{\circ}$  of north latitude and the tropic of Cancer.\*

From the course of the sun, the temperature will diminish more rapidly as we go north, than south from this belt. With this high temperature there is also a high dew-point. Humboldt has established the general fact (*Travels*, vol. ii. p. 88), that the humidity of the air approaches saturation as we advance to the equator; and that, after getting within the tropics, the dew-point rises much more rapidly than the thermometer. The same fact has been verified by Forry (*on Climate*, p. 113), and the tables of M. d'Aubuisson. The former has also shown that the atmosphere is more humid along the sea-coast, and in the vicinity of tide-water streams, than in the elevated regions of the interior, even when the temperature is the same (p. 46). In Key West, it is so loaded with humidity that books and other articles are soon covered with a greenish mould, and provisions spoil in a short time (*Ticknor on the Fever of Thomson's Island*, in *N. A. Med. and Surg. Journ.*, 1827). A similar state of the atmosphere exists in the West Indies,† and places bordering on the Gulf of Mexico, also on the western coast of Africa. Captain Alexander states, that during his voyage to the river Gambia, the hygrometer stood at  $70^{\circ}$ . In the Bight of Benin, it stood at  $79^{\circ}$ .

Bearing these facts in mind, in connection with the effects of a high temperature and dew-point on the system mentioned before, let us examine into the locale of yellow-fever. We find it in its most malignant form in the West Indies, along the gulf coast of Mexico, as at Tampico, Vera Cruz, &c.—all of which lie within the belt defined above as experiencing the greatest degrees of heat. We find it also extending south of this, as far as  $5^{\circ}$  of north latitude, and north, to New Orleans, Mobile, and Charleston. It also prevails as an endemic on the western coast of Africa. But, although its locale there is without the above limits, yet thermometrical observations show a temperature equal, if not greater than that of the West Indies.‡ It has also been met with in Baltimore, Philadelphia, and New York; but only when the temperature of these places was equal to that of those visited by it—the average being  $80^{\circ}$ .

It has been urged by the advocates of the tellurian origin of yellow fever, in support of their views, that the disease is not met with on the eastern coast of Africa, or in the East Indies, though in the same latitude with the western coast and the West Indies, where the disease is so prevalent. The difference in effect, say they, must be owing to some difference in the soil. But this argument is more specious than solid. Very little is known of the eastern coast of Africa. While the western coast carries on an extensive

\* Maltebrun, for the sake of description, subdivides the torrid zone into three. Of these, he says "the equatorial zone, properly so called, is temperate in comparison with the zone of the tropic of Cancer, which contains the hottest and least habitable regions of the earth." *Universal Geog.*, lib. xvii. p. 148.

† "The moisture of the atmosphere is so great that iron and other metals easily oxidated, are covered with rust. This humidity continues under a burning sun. The inhabitants live (say some writers) in a vapour bath." (Maltebrun's *Universal Geog.*, lib. xciii.)

‡ "The sea-coasts of this region (the tropical region of Western Africa), experience the most intense heat that is known in any part of the globe. The cause of this is to be found in the east winds, which arrive on these coasts after having swept over the burning surface of Africa in all its breadth. \* \* \* Of all the countries of Western Africa, the Gold Coast seems to be most subjected to most intense heat. Near the Rio Volta, Isert saw the thermometer of Fab. rise to  $95^{\circ}$  in an apartment, while it was at  $134^{\circ}$  in the open air." (Maltebrun's *Universal Geog.*, vol. ii. lib. lxvi. p. 68.)

trade with European nations in gold-dust, ivory, and slaves, and is also the seat of two flourishing colonies; the eastern, in the same latitude, is seldom visited by Europeans. So little, indeed, is known of it, that the existence of the Lupata mountains, a chain laid down on most maps as running parallel to the coast, still admits of dispute among geographers. The Portuguese settlement of Mozambique, situated between  $10^{\circ}$  and  $20^{\circ}$  south latitude, is the only part occupied by Europeans. And, although we have no accurate description of its climate, yet we have the general fact stated, that it is so unhealthy that "at an average of one hundred soldiers, seven only survive a residence of five years." (*M'Culloch's Geograph. Dict.*, art. Mozambique. See also *Julia sur l'Air Marecageux*, p. 14.)

But even were the fact established that the disease is not met with on the eastern coast of Africa or in the East India Islands, though situated in the same latitude with the West Indies, Vera Cruz, &c., it does not militate against the theory here advanced. The same latitude does not imply the same temperature. Temperature is affected by other agencies, as the nature of the soil, the prevailing winds, the quantity of moisture, the electrical state of the atmosphere, elevation, and the physical character of the adjacent countries and seas. The influence of these is very great. Thus Mr. Webb found the snow line on the southern declivity of the Himalaya mountains, at an elevation of 13,000 feet; while on the northern declivity, *though a degree further north*, the snow line ascends to 16,000 feet. Here they more than neutralize the effects of one degree of latitude. The southern declivity rises abruptly from the low plains of the Ganges. The northern, though steep, does not descend to a low country, but terminates in an immense plain, the surface of which is 10,000 feet above the level of the sea. The radiation from this surface is the cause of the difference in temperature. (*Penny Cycloped.*, art. Climate.)

The temperature of the countries of Western Europe differs by  $8^{\circ}$  or  $10^{\circ}$  from those on the eastern coast of our own country in the same latitude. A similar difference is found to exist between our Pacific and Atlantic coasts.

The discrepancy between parallels of latitude and isothermic lines is further shown by the observations of Humboldt. His isothermal line of  $55^{\circ} 40'$  Fahr. passes near Philadelphia in latitude  $39^{\circ} 56'$ ; on the Pacific coast it is found at Cape Foulweather, a little south of the mouth of the Colombia river; near Pekin in China, latitude  $40^{\circ}$ , and near Bourdeaux in Europe, latitude  $45^{\circ} 46'$ . The isothermal line of  $32^{\circ}$  shows a still greater difference. It passes between Uleo and Enontakies, Lapland, situated respectively in latitude  $66^{\circ}$  and  $68^{\circ}$ ; and through Table Bay, Labrador, in latitude  $54^{\circ}$ . These lines were run on the *annual* temperature. Those of seasons differ as much. Thus the isothermal line, (of equal summer temperature,) as ascertained from data furnished in the *United States Army Meteorological Register*, passes through Key West, latitude  $24^{\circ} 33'$ , and Fort Gibson, latitude  $35^{\circ} 47'$ . (*Ferry on Climate.*)

But although they have not the yellow fever in the East Indies, yet they have very severe forms of bilious fever—as the jungle-fever and the Bata-vian fever; the latter of which it would be very difficult to distinguish from yellow fever. The one, as described by Mr. Shields, surgeon in the British Navy, and the descriptions of other physicians in the southern part of our own country and in the West Indies, when compared, show that they are nearly, if not altogether, identical.

In order to test the accuracy of the opinions I had formed in regard to the cause of intermittent fever, I instituted the following experiment. As

mentioned above, the disease was prevalent every Spring and Fall where I lived. About a mile distant there was another plantation where it was unknown. I thought that if these views concerning the origin of the disease were correct, the daily oscillations of temperature of the two places would show a difference corresponding to that exhibited in point of health. At the beginning of the month of April I procured two thermometers exactly alike in appearance, made by the same manufacturer; and after trying them in fluids of various degrees of temperature, to make sure that they agreed, I left one at the place exempt from the disease, and got a member of the family to mark it immediately before sunrise, at twelve o'clock M., three P.M., sunset, and at ten o'clock at night. I kept the other myself, and marked it at the same hours.

A record of most of the cases of the disease as they occurred was also kept. Owing to the length of the tables thus obtained, and their being somewhat imperfect, they are here omitted. As far as they go, however, they establish the fact, that the temperature was higher at three o'clock P.M., and lower at sunrise and ten o'clock at night, at the place subject to the disease, than at the place exempt from it; and that this difference was greater in April, May, and the early part of June, than in the latter part of June and July.

Up to the 20th of April, the thermometer was on one occasion only as high as 70°, and only thrice above 60° Fahr. Below 60° the temperature is not sufficiently high to lessen materially the demand on the heat-generating apparatus within the system. It has still to produce enough of heat to keep the temperature of the body 38° above the surrounding atmosphere. Oxygen, from the part it plays in this process, must still be taken into the system in considerable quantities; and the lungs, being the organ by which this is effected, continue, comparatively speaking, in reference to the liver, the stimulated organ. If a person, under these circumstances, be exposed to cold of sufficient intensity to cause internal congestion, the brunt of the congestion will fall on some part of the respiratory apparatus, and he will have an attack of catarrh, pleurisy, bronchitis, or pneumonia—diseases quite common at this season.

From the 20th to the end of April, the thermometer, with one exception, was above 70°, and half of the time above 80°. The demand on the heat-generating apparatus within the body is evidently much lessened. It has now to produce only enough of heat to keep the temperature of the system about 20° above the surrounding medium. Less oxygen will, therefore, be taken in, and consequently less carbon thrown off as carbonic acid from the lungs than in the former case; and there will be more of the decarbonizing labour thrown on the liver. It now becomes stimulated. If, under these circumstances, a person be exposed repeatedly to a low temperature, as was the case here in the mornings and evenings at the place subject to the disease, so as to produce internal congestion, the liver will bear the burden of it; and he will have an intermittent or some other disease of hepatic origin. Accordingly we have several cases occurring about the last of April, and the beginning of May. After the first of June the temperature at sunrise was below 60° in a very few instances only. It was at these times that the cases of disease met with after this period occurred. The range of the thermometer at sunrise and ten o'clock, P.M., during June and July, with very few exceptions, showed a temperature not low enough to repel the blood from the cutaneous surface on the internal viscera. In July it averaged 70°.

I was prevented by circumstances from continuing the table through August, September, and October, which I very much regret. From the prevalence of the disease during these months, a table of their temperature would be of great importance. The range of the dew-point ought also to have been kept; as the effects of both extremes of temperature are much increased by humidity of the atmosphere. But I was prevented from doing this on account of the difficulty of obtaining ice.

An interesting question now presents itself, viz.: how can differences of temperature so marked be accounted for in places at such a small distance apart? This is readily explained by a little attention to their topography. The place subject to the disease is situated on the east side of, and about 150 yards from, the James river, on a hill about 50 feet above the level of the stream. Between the base of the hill and the river is a level piece of ground, with a sandy alluvial soil, about 60 or 70 yards wide. The soil, too, all around the house partakes strongly of the sandy character. The banks of the river are precipitous, and the difference between high and low water-mark is only about four feet. The place exempt from the disease is situate on a higher hill, and three quarters of a mile farther from the river. Extending all around between it and the river is a piece of woods having a thick undergrowth. The soil is argillaceous. Now we know that sand is a good radiator but a bad conductor of heat. During the action of the sun's rays the sandy soil at the place subject to the disease radiated more caloric than the argillaceous soil at the place exempt from it; consequently, the temperature at the former place was the higher during the day. But the sandy soil, being a bad conductor, was heated in the daytime to a very limited depth only, and after sunset its high radiating power brought its temperature down rapidly, so that in a short time it ceased to impart caloric to the superincumbent atmosphere. At the same time the river continued to absorb heat from the air until both became of the same temperature. A low temperature at night was the consequence. On the other hand, at the place exempt from the disease, the soil being a better conductor became heated during the day to a greater depth; it therefore retained its heat longer, and continued to impart it through the night to the air; and the temperature was thus kept higher than at the former place.

This reasoning derives confirmation from the experiments of Sir Humphry Davy. He found that when soils are perfectly dry, those that most rapidly become heated likewise cool most rapidly. A rich black mould, which contained one-fourth vegetable matter, had its temperature raised in an hour from 65° to 88° by exposure to sunshine, while a chalk soil was heated only to 69°. But the mould, being removed into the shade where the temperature was 62°, lost 15°; whereas the chalk, under the same circumstances, had lost only 4°. Alluvial and clay soils show similar results.

The experiments of Davy are important in another respect. They enable us to account for the prevalence of these diseases in places the soil of which is rich in animal and vegetable matter. This fact has been long familiar to the profession; and has led to the belief by many, that they are caused by a poison generated in the process of vegetable and animal decomposition. In the early part of this essay we showed the fallacy of such a doctrine. Davy found, that, of the ingredients of soils, animal and vegetable matter facilitates most the diminution of temperature; also that this property is much increased in all soils by the addition of moisture. A clay soil previously dried was artificially heated to 88°, and then exposed

in a temperature of 57°. In half an hour it had lost only 6°. But an equal portion of the same soil, containing moisture, after being heated to 88°, and then exposed in a temperature of 55°, was found in one quarter of an hour to have attained the temperature of the room. Now the soil of nearly all places bordering on tide-water streams is rich in these two ingredients. A high temperature during the day and a low one at night are thus produced. Accompanying this there is also a humid condition of the air. "The atmosphere in the neighbourhood of currents of water becomes much more highly charged with aqueous vapour than that of the uplands." (*Essay on the Influence of Water on Temperature of Soils, in Journ. of the Royal Agricultural Soc. of England*, by J. Parkes, Esq., Chief Engineer to the Soc., 1844.)

This is verified by the observations of Professor Gardner. He found the air, four feet above a sheltered marsh where he made his observations, in the height of summer to be so fully charged with the vapour of water during the day, as to be within a few degrees of saturation, and surcharged with vapour at night. From observations made on an adjoining hill, 700 feet distant, and elevated 150 feet, the air exhibited a dryness of 12°, 16°, and 21°, above the marsh. (Gardner on the "Dew-point," in *Am. Journ. of Med. Sciences*, January, 1846.) The heavy dews of low places prove the same fact. This humid condition of the atmosphere increases the effects of the high temperature during the day by interfering with the pulmonary and cutaneous transpiration, and of the low temperature at night, by its being a better conductor of caloric than a dry one. A temperature that would be perfectly harmless to vegetation when the air and the plant both are dry, will produce frost after a rain and when the air is moist, and thus become injurious. Of this fact every farmer is well aware. The difference in respect to night temperature between places near, and those remote from, streams of water, is further shown by what takes place on the occurrence of the late frosts of the Spring, and the early frosts of the Fall. Near the streams they are always more severe than at a distance, so that plants in the former situations are often killed, while in the latter they escape unhurt. Similar is the effect on the animal economy. The severest forms of the disease are always met with along the river courses; and in southern countries, in such situations, it assumes the type of yellow fever; while in the interior it presents the severe remittent form. As we proceed north the remittent becomes the prominent type along the bodies of water, and gradually becomes lost in the intermittent as we recede from them towards the uplands, until at last the disease disappears.

To the diseases already mentioned as being produced by these causes may be added dysentery. Congestions of the hepatic vessels must gorge the veins that combine to form the vena portarum. From the enlarged spleen produced by intermittents of long standing, we have reason to infer, that in slight congestions the splenic vein is the one chiefly involved. Whatever gorges the splenic vein must gorge its tributary, the inferior mesenteric, which carries the blood from the rectum and the descending colon. On this congestion of the mucous membrane inflammation is easily lighted up. As in fevers the congestion may exist in various degrees from that which produces the mild intermittent, to the severest forms of yellow; so here we have its effects varying from a diarrhœa to the severest forms of sporadic cholera.

The identity of the causes of dysentery and intermittent fever is held by most writers on the diseases of tropical climates. They attribute both

to *malaria*. Both diseases prevail at the same season of the year: dysentery among the hills and elevated regions of the interior, and intermittents and remittents in the low, level countries along the sea-coast. In many places they appear together, the one running into the other. (*Ferry*, p. 220; *U. S. Army Med. Stat.*, p. 27; *Johnson on Tropical Climates*, &c. &c.)

We are now able to explain the occurrence of the disease in the situations described by Dr. Ferguson, on which he founded his theory. He says:—

“In the month of June, 1809, our army marched through a singularly dry, rocky, and elevated country on the confines of Portugal, the weather having been previously so hot for several weeks as to dry up the mountain streams. In some of the hilly ravines that had lately been water courses, several of the regiments took up their bivouac, for the sake of being near the stagnant pools of water that were still left among the rocks. Several were seized with remittents before they could leave the bivouac next morning, and that type of fever continued to affect the portion of the troops, exclusively, which had so bivouacked for a considerable time.” \* \* \*

“The army advanced to Talavera, through a very dry country, and in the hottest weather fought that celebrated battle which was followed by a retreat into the plains of Estremadura, along the course of the Guadiana river, at a time when the country was so arid and dry for want of rain, that the Guadiana itself, and all the smaller streams had, in fact, *ceased to be streams*, and were no more than *lines of detached pools* in the courses that had formerly been rivers; and there they suffered from remittent fevers of such destructive malignity that the enemy, and all Europe believed, that the British host was extirpated; and the superstitious natives, though sickly themselves, unable to account for disease of such uncommon type amongst the strangers, declared they had all been poisoned by eating the mushroom (a species of food they hold in abhorrence), which spring up after the first Autumnal rains, about the time the epidemic had attained its height. The aggravated forms of the disease differed little or nothing from the worst yellow fevers of the West Indies; and, in all the subsequent campaigns of the Peninsula, the same results uniformly followed, whenever, during the hot season, any portion of the army was obliged to occupy the arid encampments of the level country.” (*Marsh Poison*, p. 5.)

Similar instances are given by Sir Gilbert Blane. In all these, the sandy soil radiated powerfully through the day, making the heat very great; but being a bad conductor, it was heated to a depth very little below the surface, and radiation ceased soon after sunset. The pools acted like the river in the place where the observations given above were taken, and a low temperature at night was the consequence.

But these oscillations of temperature may vary in respect of each other. They may be compared to two forces co-operating to produce a given result, which may vary in the relative amount of power exerted by each. If one does less the other must do more, and *vice versâ*. So here, if the heat during the day be great and long continued, and the dew-point high, the liver and skin are much weakened by stimulation, and the disease may be produced by a change of temperature that would be innocuous if the previous stimulation had been less.\* On the other hand, if the heat of the day has not been great, it will require a greater daily depression to

\* Within the torrid zone, the system becomes so sensible to changes of temperature, that a fall of a few degrees of the thermometer is sufficient to cause the inhabitants to suffer from cold. Humboldt relates that he and his companions were prevented from sleeping by the cold, while the thermometer stood at 71° Fahr. “The humidity which modifies the conducting power of air for heat, contributes greatly to these impressions.”

produce the disease. Hence it is that two places may have the same temperature during the day; but one, by being near a mill-pond, or a sheet of stagnant water under some other name, will have its temperature at night brought lower than the other, and its inhabitants will be subject to intermittents, while those of the other place will be exempt from them.

This enables us to explain the fact so often met with, that places perfectly healthy, situated near a rapidly running stream, become sickly after the stream has been changed into a state of stagnation by having a dam thrown across it. The channel of all rapidly running streams is shallow and rocky. The rocks become heated through the day and radiate at night. The stream being shallow becomes itself heated. Also in a rapidly running current there is a great deal of friction. Unless it be an exception to everything else in physics, it must generate caloric. These three combined, must impart considerable heat to the superincumbent air during the night. But when the stream becomes stagnant they are all arrested.

We can also see why the disease is more apt to attack individuals inhabiting the first floor of a house than those in the second. The temperature at night is always lower in the former than in the latter. Dr. Wells found a difference of from  $7^{\circ}$  to  $12^{\circ}$  in this respect. The following is from a tabular view of his observations on this subject, made on the evening of the 13th of May, 1813:—

	6 h. 45 m.	7 h.	7 h. 20 m.	7 h. 40 m.	8 h. 45 m.
Heat of the air 4 ft. above the grass	$60\frac{1}{2}^{\circ}$	$60\frac{1}{2}^{\circ}$	$59^{\circ}$	$58^{\circ}$	$54^{\circ}$
“ “ on the grass plat	53	51	$49\frac{1}{2}$	49	42

Our own observations found the difference between the upper and lower floors of a house two stories high, to vary from  $6^{\circ}$  to  $10^{\circ}$ . The effect of this difference is much increased by the greater humidity of the air near the ground. The walls of rooms on the first floor we have seen covered with a heavy deposit of dew, while the rooms above were perfectly dry.

This enables us, also, to account for the prevalence of the disease in the outskirts of large cities, and along the wharves; while in the centre, where the streets are paved and thickly built, it is rare. In the former situation, from the sparseness of the houses and the absence of the pavements, there is comparatively little heat imparted to the air by radiation after sunset. Also the contiguous river and the pools of stagnant water always met with in such situations absorb caloric. A reduction of temperature is the consequence. Whereas, in the centre of the city, the houses and pavements becoming heated during the day, radiate through the night, and thus a low temperature is prevented.

New countries, when first brought under cultivation, are often fertile sources of these fevers, but become healthy as the country becomes more improved. It may be thus accounted for. In all new countries there is a predominance of forests. Indeed, in some places, the settlements, in comparison to them, in extent are like islets to the ocean. The surrounding wood interferes much with that circulation of air so refreshing and so much needed during the day in the summer season. The heat of such situations is therefore greater than that of a clear, open country. The air, also, being in a state of partial stagnation, becomes more humid. The temperature of the forest, at the same time, is kept low by evaporation from the surface of the leaves and by the foliage, preventing the caloric rays from reaching the ground.

(*Travels*, p. 402.) The same fact is mentioned by Bouguer, in reference to the climate of Martinico (*Fig. de la Terre*), and by Captain Bligh. (*Voyage to the South Seas*, pp. 265, 316.)

It is a fact well known that snow lies longer in forests than in the open country adjacent. It is remarked by Umfreville that, at Hudson's Bay, the ground in open plains thaws to the depth of four feet, and in the woods to the depth of only two. Moreover, it has been determined by thermometrical experiments, that the temperature of the forest at the distance of twelve inches below the surface of the earth is, compared with an adjacent open field, at least  $10^{\circ}$  lower during the summer months; while no difference is observable during the season of winter. After sunset, the surrounding woods act here like the pools and streams of water afore-mentioned. This action is assisted by the soil of such places containing much vegetable matter, a substance which, Davy has shown, parts with caloric very readily. A low temperature at night is thus produced. It is easy to see how repeated attacks on the domain of the forest, and continued cultivation, remedy the evil.

We can also understand how indiscretions in diet, and the depressing passions, as fear, grief, &c., aid in producing the disease. "Congestion of the portal system is liable to occur when the alimentary canal is distended with food." (*Carpenter's Physiology*, p. 543.) The depressing and perturbing passions, as fear, grief, anxiety, cause the blood to recede from the surface, as is shown by the ashy paleness which they always produce.

Over-indulgence in rich food, containing much fatty matter, likewise predisposes to these fevers. Fat contains 80 per cent. of carbon, peas and beans 37 per cent., potatoes 12 per cent., and bread 30 per cent. Indulgence in rich animal diet in warm weather, when there is so little need for the consumption of carbon for keeping up the temperature of the body, will have the effect of throwing more labour on the liver. It will be stimulated; and, as a consequence, rendered more susceptible to the influence of the diurnal changes of temperature. A vegetable diet, on the other hand, contains comparatively little carbon, while it is rich in protein, the proximate element of all the tissues. In the warm season it will obviously be more conducive to health than the other.

Another interesting fact connected with these diseases is, that they are arrested by a heavy frost. A little attention to the effects of frost on the air enables us to account for this. The humidity of the atmosphere of such situations during the sickly season has been already mentioned. This is precipitated by frost. The air then becomes a bad conductor of heat and electricity, and has a bracing effect on the system. Fires are now made up night and morning. Winter clothing is put on. All these coöperating, prevent that internal congestion from which the disease takes its origin. A frost not heavy enough to make the inhabitants thus act on the defensive, never arrests the disease. All writers on the subject, as well as the residents of those districts where it prevails, agree on this point.

In accordance with the theory here entertained, is the popular prophylactic measures universally recommended in malarious regions. Popular belief is generally founded on correct observation. If a stranger goes into a neighbourhood where the disease is prevalent, and asks what measures he must take to escape it, he will be told by every one to avoid the hot suns by day, and the cool, damp air by night; not to expose himself after sunset, nor before the sun has risen high enough to dispel the chilliness of the morning air: or, if he has to expose himself at the dangerous hour, to see to it that he is properly protected by additional clothing. All writers on the subject recommend the same thing. "A person may, I believe, sleep with



perfect safety in the centre of the Pontine marshes, by having his room well heated by a fire during the night (*Sir J. Clarke, Sanative Influence of Climate*, p. 117). The utility of fires has been mentioned also by Pliny, who quotes the authority of Empedocles and Hippocrates to the same effect. Lancisci points it out at Rome, and Napoleon used them very largely and with success when his armies were occupied in the worst districts of Italy. It was also resorted to in Africa. "A superintendent engaged in directing the cutting of wood erected thirty earthen furnaces on the spot where his men were at work, lighting them every day. Before this he had always from forty to forty-eight of his workmen sick; when, in a short time they were reduced to twelve, then to four, and finally to one" (*M'Culloch on Malaria*, pp. 285-6).

I proposed the following two questions to an officer of our navy who had spent between two and three years in the Gulf of Mexico. 1. What measures were adopted, during your connection with the Gulf squadron, to protect the men against fever? 2. Was there any difference in the habits, manner of living, &c., between those who had the disease and those who had not? If so, state it.

In reply to the first, he said that orders were given to the men every evening at, or immediately before sun-set, to put on their flannel jackets; to the second, that they who neglected to put on their jackets had fever, while they who complied with the precautionary orders escaped, generally speaking.

Sir James Clarke, in his work above referred to, in describing the climate of the different places, generally has given the daily range of temperature and the prevalent diseases. Wherever the former is great, malarious fevers are given among the diseases. The reverse is the case where it is small. Thus at Pisa, Naples and Rome the daily range is great, and intermittents and dysentery are prevalent; while at Madeira and New South Wales, where the daily range is small, they are unknown.

It has often been observed that the crews of vessels lying at some distance from a malarious coast, if they sleep on board of the vessel, remain healthy; while those of them who may happen to remain over night on shore are attacked by the disease. This has been explained by the disciples of the miasmatic school by saying that the poison is destroyed by passing over a body of water. As to the extent of water necessary for this purpose they are not agreed. Sir John Pringle and Sir Gilbert Blane found eight or nine hundred yards sufficient in the neighbourhood of Walcheren. The former, however, found five miles not enough on another occasion. Lind makes three miles the maximum to which it will travel; while M'Culloch thinks it may be carried from Holland to England, or even to Scotland. A more philosophical explanation of this fact is furnished by a reference to the temperature of the sea and of the land. The diurnal extremes of temperature of places bordering on bodies of water has already been mentioned. But the case is different at sea. Humboldt remarks that "in the Atlantic between 11° and 17° of latitude, the greatest variations of heat rarely exceeded 1.5° or 2°; and I often observed that from ten in the morning to five in the evening the thermometer did not vary 0.8° of a degree. In looking over 1400 thermometrical observations made during the voyage of M. Krusenstern in the equatorial regions of the South Sea, we see that the temperature of the air changed from day to night only 1° or 1.3° cent." (*Travels*, vol. ii. p. 57.) M. Peron says that "everywhere the sea is colder at noon and warmer at night than the surrounding air." (*Annales du Museum*, tom. v. pp. 123-148.) This is in harmony

also with the experiments of Ellis, Foster, and Irvine (*Humboldt*, vol. ii. p. 71).

The dew-point over the sea resembles that of high and dry places in the interior of the country. This was proved by the observations of Captain Sabine during his voyage to the tropics to ascertain the length of the second's pendulum. His register commences on the 20th of February, 1822. During this voyage three measures were made daily, viz. at 8 A. M., 1 and 6 P. M. The observations were continued on the Gambia and Rio Grande rivers. The mean temperature was found to be  $70^{\circ}$  Fahr. and the mean dew-point  $57.7^{\circ}$ ; hence the average drying power was  $12.3^{\circ}$ .

Compare these observations at sea with those made on shore at Sierra Leone from the 25th of February to March 22d. These give  $78.85^{\circ}$  as the average mean of the thermometer, and  $72.7^{\circ}$ , as the average dew-point; so that the average drying power here was but  $6.15^{\circ}$  or only half of what it was on the adjacent seas.

From this it appears that at sea there is a more equable diurnal temperature and a drier atmosphere than there is on shore; hence one is healthy and the other unhealthy. But if a vessel be near the shore, and the wind blowing from the land, the persons on board experience more or less of the land climate according to their distance from the shore; and with it the land diseases.

That the cause here advocated is of itself capable of producing the disease, most writers on the subject admit. "Some have supposed that all cases of intermittent have the same origin. Experience, however, is opposed to this opinion. We every now and then meet with instances which can by no possibility be traced to miasmata." (*Wood's Practice*, vol. i. p. 239.)

"We see then this important agent (malaria) greatly varying in force, and from standing occasionally the unaided principle, the *instar omnium*, in the production of fever, dwindle away until it can be scarcely distinguished among the *auxiliaries*.

"Such being the case, is it not probable that when the latter are numerous or powerful, they may in some instances induce the aforesaid disease without the assistance of marsh exhalations?" (*Johnson on Trop. Clim.*, p. 122.) "Soldiers and others have been attacked and died of yellow fever before they landed in the West Indies or could be exposed to the influence of the land miasmata in any shape." (*Ferguson*.)

The experiment of M. Brachet is important and interesting in reference to this. Towards the close of October in the year 1822, when the water was cold, he bathed at midnight for several successive nights in the river Saone. After each bath he betook himself to a warm bed, and in a short time was affected with a considerable reaction which terminated in a perspiration. At the end of seven days he omitted the practice, but was, nevertheless, affected nightly with a regular intermittent paroxysm. This continued for a week, but as he was tolerably well during the day, he determined not to interfere with it. On the seventh night he was summoned before midnight to a woman in labour. The ride to her house heated him, and on his arrival there he kept up the heat by placing himself before a large fire, and from that time the febrile phenomena ceased.

This experiment proves the theory almost to a demonstration.

But while it is acknowledged by those who hold the theory of a tellurial origin, that the causes here advocated may and do produce the disease, it is likewise admitted that the miasm may lie dormant in the system for months, yea for years, if not called out by exposure to extremes of tem-

perature or by some other of the "auxiliaries." If it may lie dormant for years, what is to hinder it from remaining so for life? If then, the causes here entertained are sufficient of themselves to produce the disease; and if the miasm is not able of itself to produce the same effect, is it logical—is it philosophical—to make miasm *the cause*—the "*instar omnium*"—of all the mischief?

The periodicity of intermittent fever has given rise to much curious theorizing, which we need not here examine.

In the early part of this essay we have shown that the circulation is more active in the portal system in summer than in winter; also that it is subject to periodic augmentations every day after each meal. We have also shown how a high temperature, long continued, produces debility of the liver, and how this debility is increased by the congestions produced by repeated exposures to the night air, which, in miasmatic districts, is always cool and damp. Under the continued action of these agents this debility goes on increasing: the tonicities of the organ decreasing in the same proportion, until at last, at one of the periodic determinations of blood to the part, the tonicities give way, and the blood collects in abnormal quantities in the portal vessels, and gives us the phenomena of a chill. A chill is, therefore, nothing more than the natural periodic determination of blood to the part, taking place in excess. The time at which it most generally occurs corroborates this view, viz.—in the morning, about noon, or in the afternoon. This is the rule: but as the human body is exposed to a number of deranging influences, and people differ much in their habits; so we every now and then meet with cases which exhibit a deviation from it. We can thus account for the occurrence of the first paroxysm. A recurrence of the chill is prevented by the fever that follows, so long as it lasts. But it having passed off, and the atony of the liver and portal vessels remaining, we might expect a return of the ague fit at any of the regular periods of determination of blood to the part. Here, however, we find another law of the disease which we are unable to explain. We cannot tell why a paroxysm occurring in the morning has a disposition to return the following morning, and thus give us a *quotidian*; while one occurring about noon is apt to pass over a whole day, and reappear on the third at the same hour, giving us a *tertian*; while one occurring in the afternoon will pass over *two* days, and thus give us a *quartan*. A solution of this must be sought for in the physiological laws of the organ involved. Our knowledge of these, however, is very imperfect; and so long as it is so, any attempt to explain its pathological laws must end in failure. The one is only a modification of the other.

If the congestion produced by the first paroxysm has been sufficiently great to excite inflammation, the febrile condition of system continues; but if the inflammation be not extensive, the organ still acts, to a certain extent, in accordance with its physiological laws; and the disease presents remissions and exacerbations corresponding somewhat to the paroxysm and pyrexia of the intermittent. We have thus the phenomena to which the term *remittent* is applied. But if the lesion produced by the congestion be very extensive, the derangement of system is so great as to set aside entirely the physiological laws of the organ involved; and we have the *continued* form of the fever.

In conclusion, we give the following as a summary of our views on this subject:—

1. That the lungs and the liver are the great decarbonizing organs of the

body ; that, their function being complementary, the activity of that function is always in an inverse ratio.

2. That during winter, the lungs, from the part they perform in the generation of animal heat, are the more active organ. On the other hand, in summer, the liver is the more active.

3. That exposure to a low temperature repels the blood from the surface to the internal organs. If this exposure takes place in winter, the lungs being then the active organ, the brunt of the congestion falls on some part of the respiratory apparatus: hence, we have bronchitis, pleurisy, pneumonia, and catarrh, as the prevailing diseases.

4. But if this exposure takes place in summer, the liver being then in a state of stimulation, the force of the congestion falls on it. It becomes deranged, involving all those organs, more or less, whose blood has to pass through the liver to reach the heart. Hence, we have bilious affections, as intermittents, remittents, dysenteries, &c., as the prevailing diseases.

5. That the pulmonary diseases of winter and of cold climates, and the hepatic diseases of summer and of warm climates, are both produced by the same agents acting on the system. The different effects being solely owing to the different modifications of the agents, and the different conditions of the system when exposed to their influence; and we might with as much truth say that malaria was the cause of the one as of the other.\*

Nature is simple in all her operations. It is only in proportion to our ignorance that she appears mysterious. The mystery that all admit to hang over the origin of these diseases, and the failure that has hitherto attended every effort to explain it satisfactorily, we think is solely owing to the spirit of inquiry being on the wrong track. The idea of a specific poison, first started by Lancisci, has been embraced by most writers on the subject ever since: as a consequence, their labours have been expended in tracing the origin of a thing, the very existence of which is hypothetical.

Of the truth of this, the Proteus-like appearance that this agent assumes in the hands, or rather the heads of different writers on the subject, is *primâ facie* evidence. Thus one describes it as being softened or entirely disarmed by passing over 800 yards of water. (Sir G. Blane.) Another thinks it can safely perform the voyage from Holland to England, yea, even to Scotland, a distance of not less than four hundred miles! (M<sup>r</sup> Culloch.) It cannot ascend to the second story of a house, and yet can seize its victim on a mountain side four hundred feet high. (Ferguson.) Stygian-like it ascends from the bowels of the earth, and angel-like descends with the dews from heaven. Were it delineated with all its eccentricities attached, I doubt much if even its most devoted admirers would be pleased with the picture.

*Note*—I take this opportunity of expressing my obligations to Dr. Heiskell, acting Surgeon General, for copies of the *Army Meteorological Register*, and of the *Army Medical Statistics*; to Lieut. M. F. Maury, of the National Observatory, and to W. C. Bond, Esq., of the Observatory of Harvard University, and to Professor Phillips, of the University of North Carolina, for valuable meteorological tables; also to Dr. Geo. Metcalf, of Mendon, Mass., for meteorological tables, and statistics of disease. Owing to the series of tables not being complete, I have postponed drawing on the valuable information they contain, for the present. On a future occasion I may resume the subject.

\* Since this essay was prepared, Dr. Hays pointed out to me a paper by Dr. Bell, of this city, in which nearly the same views are advocated. I have since read the paper, and feel strengthened by the thought, that another, differing much in the manner of investigating, should be led to the same result.